

On Some Taxonomic Categories which have been Proposed for the Classification of Large Gastropod Genera

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IN A RECENT ISSUE of this journal, SCHILDER (1966) gave some "Personal Views on Taxonomy" in relation to classification at the generic, and lower, levels. It was suggested by the editor that the *Veliger* could provide a forum for the discussion of these ideas, without passing judgment. I am, therefore, pleased to present a contrary point of view. The opinions expressed here are my own, and are not necessarily endorsed by the editors; moreover, I do not claim to have originated them, for the taxonomic principles at issue are discussed by SIMPSON (1961) and others. Yet it does seem reasonable to re-emphasize these points, in view of the practices of many malacologists.

The issue at hand has been characterized as a difference in taste between "splitters" and "lumpers" (SCHILDER, 1963). But the basic problem is somewhat more involved; indeed, it is highly technical. The fundamental principles of taxonomy include some strictly logical features which cannot be left out of consideration in practical systematics; it is these which shall be discussed here.

Among the basic concepts which have been elaborated in taxonomic theory is the distinction between *taxa* and *categories*. A *taxon* is a group of animals; *Cypraea* is a taxon. A *category* is a level in a system of classification; the genus is a category. SCHILDER (1966) maintains older usage by not distinguishing between the two. The introduction of the distinction makes it easier to avoid a number of mistakes, particularly with respect to problems of definition. It is one thing to define the name of a category, such as "species," another to define the name of a particular taxon, such as "Gastropoda" (GHISELIN, *Systematic Zoology*, in press). Modern biology (*e.g.* MAYR, 1963) generally uses the word "species" to designate populations of organisms which are reproductively isolated from other such groups. Thus, two organisms, by definition, belong to different species if it is impossible, under natural conditions, for them or their descendants to interbreed. This is what is meant when a biologist, rightly

or wrongly, says that all men constitute a single species. Yet as all working systematists are abundantly aware, it is in practice difficult to tell what is, or is not, by definition a species. Therefore, indirect evidence, such as distribution and anatomical structure, is used to erect tentative classifications until definitive evidence is available. SCHILDER (1966, p. 183) would seem to agree on these points. Yet it is very misleading when he calls a morphological test a "definition": "groups of similar shells should be treated as different species if they can be separated by at least one well recognizable character showing no intermediates even in extreme specimens." To call this criterion a definition implies that should evidence be brought forth which demonstrates that a given grouping of organisms does exchange, or can exchange, genes with another such grouping, they would still belong to different species. Thus, by definition, one would have to subdivide the species which includes domestic dogs into at least two species, including one for basset hounds, as these show no intermediates in their short-leggedness.

The objection to using a morphological species definition is that it seems desirable to have the classification-system correspond to an order having real existence in nature. Whether or not we happen to be able to separate specimens into groups has no bearing upon what the animals are doing in nature. It is for this reason that, in order to insure nomenclatural precision, the names of a species are defined ostensively¹, by reference to a type specimen. That is to say, a species name applies to those organisms which belong to the same species as the type specimen. The definition being ostensive, the characters, morphological or otherwise, are not defining of the name, even though they may be useful evidence.

¹ An "ostensive definition" is one in which the meaning of the word is given by stating the word and showing the thing: a christening is an ostensive definition.

The criterion of reality of taxa (*not* categories) is met upon the higher levels by having the taxa correspond, approximately, to groups delineated on the basis of their genealogical affinities; i. e., the most closely related forms are grouped closest together. At the species level, the names of taxa designate reproductively-isolated populations. At lower levels, the problem is somewhat more difficult, but in general, species are divided into subspecies on a basis analogous to the division of genera into species; subspecies are populations, but their reproductive isolation is only partial.

The manner in which organisms are classified is partly determined by the way in which groupings are organized into a unitary system. Organisms are placed into a system of classes forming the *Linnean hierarchy*, a kind of organization which has a very definite structure. As a consequence of this structure, there are certain limitations upon the manner in which groupings may be subdivided. For instance, the taxa are assigned particular *categorical ranks*: e. g., Mollusca has the rank of a phylum. The question of the precise categorical rank of a taxon (for example, whether *Cypraea* should be a genus or a family) is to some measure subject to the personal choice of systematists. However, if different criteria of rank are used in different taxa, the system becomes less informative. Further, the decision as to what rank to give a taxon is generally dependent, not only upon the relationships of the organisms, but in addition upon the degree of modification which the organisms have undergone. Thus, on the basis of their relationships, birds could easily be made a subclass of reptiles, but as they have become greatly altered, they are given a higher categorical rank and made a class.

The possible number of categories is not limited by the structure of the Linnean hierarchy. Therefore, there can be no logical objection to SCHILDER's proposal to increase the number of categories below, or just above, the species level. In large genera which show distinct, but closely-related and little-modified subgroupings, the erection of subgenera is quite desirable. However, to raise the categorical rank of a taxon, for instance to make a subgenus a genus, simply because the group is a large one, loses sight of one meaning of categorical rank. Genera should, in so far as it is feasible, correspond to a comparable degree of difference throughout the system. Otherwise the system will not convey the amount of information about the organisms which it is able to express. Hence the opinion of KAY (1960) that the genus *Cypraea* should not be subdivided into several taxa of generic rank is in accord with the logical foundations of taxonomy.

Again, there is no logical objection to having several categories upon the infraspecific level. Minor and major

differences, as SCHILDER (1966) suggests, could be distinguished on the basis of respectively lower, or higher, categorical rank. I shall omit, from this discussion, the problem of whether or not such a procedure is practical or useful; SIMPSON (1961), however, treats it adequately.

Further properties of the Linnean hierarchy impose limitations upon how animals may be classified. For instance, the groupings must be set up so that organisms are not placed in two taxa of the same rank; i. e., a shell must be either a *Cypraea* or a *Murex*, not both. Similarly, in so far as rules are adopted for assigning a particular rank to a taxon, the rules should be applied consistently throughout the group. What this means will be brought out by examples to follow.

SCHILDER (1966) suggests that sexual forms must not be named. It is curious that he calls them "taxa," although it is not impossible, in an artificial system, to treat them as such. The logical reason for not putting males and females in different taxa is that it confuses the meaning of categorical rank. If, say, *Cypraea* were divided into two groups, males and females, with a rank of subgenus, then every species of *Cypraea* would have to be broken up into two species, each of which consisted of individuals having the same sex, and no specific name could designate groupings of organisms which were not all males or all females. Such a classification system would be utter nonsense in terms of biology. Therefore, the only way to prevent confusion, and still be able to name classes of sexual forms, would be to make the division on this basis at the lowest categorical level.

Now SCHILDER (1966) does not explain the logical objections to attaching names to classes of males and females, nor does he explain why he objects to naming classes of juveniles or of monstrosities. The reasons are, of course, analogous to those for not giving males and females different names; it would play havoc with the hierarchy. He even goes so far as to suggest (SCHILDER, 1966, unnumbered figure) that all the taxa which "must not be named" are all single individuals. This does not fit in with the conventional distinction between a class and an individual. The name of a taxon may designate a class of organisms. Classes need not have members, in which case they are *null classes* (e. g., men over 25 feet tall). However, if a name is to refer to something, it must designate an individual or a class of individuals. If the name is of an individual, such as John Smith, it is a *proper name*. If the name is of a class, such as blue books, it is a *class name*. In taxonomy, the names are proper nouns or class-names according to the manner of definition. Thus if the category "species" by definition means that all species are biological populations, the names of the species are proper

nouns, and the species themselves are, in a sense, individuals. *Homo sapiens* LINNAEUS is an individual population. However, there is no logical objection to treating taxa as class names and to defining the names of taxa as classes of individuals. Now it would be absurd to assert that those categories which SCHILDER groups as formae, such as the "forma sexualis," refer to class-names for individuals. Clearly, "male cowries" and "female cowries" likewise refer to classes of organisms and are not in any way conceivable as names for single individuals. The distinctive feature of such categories is not that the names are of single individuals but rather that the names in no sense are, like species names, proper nouns.

The real issue at hand may be elucidated by a hypothetical example. Suppose that we did allow the naming of two of the kinds of taxa which SCHILDER says, and I think rightly, must not be named. We might try to name both sexual formae and monstrosities. But should we make the monstrosity or the forma sexualis the higher category? Or, in other words, which of the following are we to prefer:

genus
 species
 forma sexualis
 monstrosity, or,
 genus
 species
 monstrosity
 forma sexualis ?

In terms of taxa, this would mean a choice like the following:

males
 "type A" (male) monsters
 "type B" (male) monsters
 females
 "type A" (female) monsters
 "type B" (female) monsters; or
 "type A" monsters
 male ("type A") monsters
 female ("type A") monsters
 "type B" monsters
 male ("type B") monsters
 female ("type B") monsters.

All these groupings could, in theory, be used to classify shells. But we see that any decision to divide upon one basis before the other is wholly arbitrary and leads to biologically meaningless classifications. In nature, monsters do not separate into units composed of only males and

only females. Nor do monsters occur as sub-units of sexual populations.

Now there is no evident difference of opinion between SCHILDER and myself, as to the folly of naming classes of sexual formae and the like. Yet there are certain analogous infraspecific taxa which he says (SCHILDER, 1966) "must" or "may" be named. I shall discuss these in his sequence of presentation.

Clines. SCHILDER says that "Clines differ from true subspecies by the far more gradual passing of adjacent taxa into each other, so that only specimens coming from opposite extremes of the inhabited areas show typical characters, while the large area between these extremes contains populations of intermediate or of mixed extremes" As SIMPSON (1961) and others have pointed out, the cline is not a taxonomic concept; that is to say, clines are not groups of organisms. A cline is a geographical gradation of characters. As a variety of characters may display different patterns of variation in the same taxon, it follows that the same organism may be a part of several different clines. Clines cannot coherently be assigned any definite categorical rank; species, subspecies, even local populations may display clinal variation, and even the most trivial differences may constitute such variation. The attempt to name clines, therefore, would be like trying to divide the class of Americans into males, females and Californians.

Morphes. SCHILDER (1966, p. 185) asserts that "morphes evidently are highly stable mutants." I can see no difference between naming sexual forms and naming morphes. For a morphe is nothing more than a class differentiated upon the basis of characters which do not intergrade, irrespective of whether or not the individuals interact, in nature, as a unit. People are, or are not, achondroplastic dwarfs, and there are no intergrades, but no taxon is erected for them. Evidently, SCHILDER has not distinguished between artificial classes of mutant individuals, and natural populations in which the individuals are mutants. The failure to make this distinction leads him into a manifest self-contradiction, when he asserts that morphes are highly-stable mutants. For in referring to a morphe as a mutant, he can only mean that the individuals which make up each morphe are mutants, for a class cannot mutate. Yet when he says that morphes are stable, he can only refer to the historical nature of the class. It seems inescapable that SCHILDER has derived much of his conception of taxonomic groupings from an elementary logical fallacy, namely, confounding classes with individuals.

Aberrations. SCHILDER (1966, p. 186) defines this category thus: "Aberrations are populations the members of which have been influenced by certain special condi-

tions of their habitat; they will be found in scattered places with similar environments." Evidently, the individuals which make up these groupings are analogous to the ecotypic variations of conventional biology. But he seems not to realize that classes of aberrant individuals do not necessarily constitute biological populations. The difficulty of incorporating the aberration into biological classification is that a single organism may be referred to more than one class of aberrations, and that these may overlap with taxa of various categorical ranks. Thus it is possible for a species to consist of several subspecies, each of which has members which display the same pattern of ecotypic variation and should, therefore, be grouped together. But this, again, would not fit in with the logic of the Linnean hierarchy. The only way to get around the difficulty would be to break the taxa up into artificial assemblages.

Upon these considerations, it seems inescapable that some of the categories proposed by SCHILDER (1966) conflict with his avowed aim to "demonstrate his opinion about the phylogeny of the various taxa . . ." This is not to say that the groupings he suggests are unimportant for biology; they constitute a very useful part of systematic descriptions. However, certain types of variation, such as those seen in ecotypes and clines, are alien to the fundamental basis of classification. Nothing but chaos is gained by attempts to force such attributes upon the structure of Linnean hierarchies.

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